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On Mr. Stone's Alterations of Bessel's Refractions.
By W. H. M. Christie, Esq.

In a paper on Mean Refractions in the *Monthly Notices* for April 1880, Mr. Stone refers to some results which I gave in a paper on the Systematic Errors of the Greenwich N.P.D.'s, read at the meeting in January 1880, but not published till the middle of December last. As there would have been obvious inconvenience in following Mr. Stone in a discussion of an unpublished paper, I have preferred to defer any remarks on Mr. Stone's criticisms till the publication of my paper in the recently issued volume of the *Memoirs*. This has been the cause of the delay in my reply to Mr. Stone.

As regards the Greenwich circumpolar observations 1857 to 1865, Mr. Stone appears to be much concerned at the discordances between his residuals and mine; but the most important difference really admits of very simple explanation:—Mr. Stone had not stated what corrections he had applied, and he has not taken note of the corrections which I have applied, as specified in my paper. To begin with, Mr. Stone has not even allowed for the difference in our adopted co-latitudes, the corrections for which would increase my residuals by $0''\cdot20$. Then there is the still more important correction to the refractions beyond Z.D. 82°, which was applied to the Greenwich observations from the year 1857. Mr. Stone, it now appears, has brought back the results to Bessel's Refractions unaltered, whilst I have used the printed observations as affected with Main's correction. I certainly derived the impression from a perusal of Mr. Stone's paper of 1867, that he had taken no notice of this important correction, and I regret that there should have been this mistake on my part. Again, there is some doubt as to the exact limits of Mr. Stone's groups, for it so happens that as regards three of them, stars fall so near the boundary as to be easily transferred from one group to the next unless account is taken of the seconds of N.P.D. as well as of the degrees and minutes. In forming my residuals, as printed in the *Memoirs*, vol. xlv., I included θ *Ursæ Majoris* in the group 30° to 37° 40′, and f_1 *Cygni* in the group 37° 40′ to 43°. In both cases the precessional motion would carry the star, in the course of a few years, from one group to the next. Now, the transposition of the last-named star from the last group but one to the last group affects the last residual by $0''\cdot47$, which is more than twice the amount of Mr. Stone's estimate of the probable error of the result.

In order to give a fair comparison between Mr. Stone's residuals and my own, I have taken the results with the new law for R—D as given in my paper (*Memoirs*, vol. xlv. p. 165), and have applied a correction of $-0''\cdot20$ to reduce to co-latitude $38^\circ\,31'\,21''\cdot80$,

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and a correction to the last residual to remove the effect of Main's correction beyond Z.D. 82°, viz. $\frac{8}{12} \times -1''\cdot06 = -0''\cdot71$ when f_1 *Cygni* is included in the last group, and $\frac{8}{9} \times -1''\cdot06 = -0''\cdot94$ when that star is transferred to the preceding group. I have then applied the differences between Mr. Stone's residuals A and B to reduce to Mr. Stone's diminished refractions and co-latitude 38° 31′ 21″·61. The comparison of Mr. Stone's work and mine stands thus:—

N.P.D. above Pole − N.P.D. below,

with Mr. Stone's Refractions, Co-lat. 38° 31′ 21″·61, *and new Law for R − D.*

N.P.D.		0°–15°	15°–30°	30°–37° 40′	37° 40′–43°	43°–45½°
Mr. Stone's values		−0″·15	+0″·10	+0″·14	+0″·08	−0″·10
Mr. Christie's „	I.	−0·37	+0·02	+0·10	−0·25	−0·14
„ „	II.	−0·37	+0·02	+0·10	−0·18	−0·61

In the residuals I. f_1 *Cygni* is included in the last group; in II. it is transferred to the group 37° 40′–43°. These results of mine have been obtained by giving equal weight to each annual result, regardless of the number of observations. In the last group, four of the results have two observations S.P., and two or more above pole, and if double weight be given to these, the last residual of I. becomes − 0″·29.

Thus it appears that by simply applying the corrections for difference in adopted co-latitude and for Main's diminution in the refractions beyond Z.D. 82°, my last residual I. is brought into substantial agreement with Mr. Stone's, and, under these circumstances, I think I have some ground of complaint in regard to the language which Mr. Stone has used respecting the supposed discordance. His statement that either his results or mine are in error is based on a misapprehension, for which he is mainly responsible, in that he gave no explanation in his paper of 1867 of the corrections which he had applied to form his residuals and that he disregarded the explanation which I gave in presenting my residuals. I may add that I had no intention of casting any doubt on the accuracy of Mr. Stone's arithmetic, and that I did not start with any idea of examining his work. On discussing the circumpolar observations for the period 1851 to 1861 I arrived at results which gave so little support to Mr. Stone's diminution of Bessel's Refractions that I was naturally led to discuss the observations 1857 to 1865, which Mr. Stone had used, and finding that they did to a certain extent support his hypothesis, I thought it only fair to say so. But in giving a comparison between his results and mine, I stated that I did not know what corrections Mr. Stone had applied, and that the different system of weights which we had respectively used might perhaps account for a portion of the discordances. I

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may remark that the difference in the R − D corrections with the old and new laws respectively affects the last residual by 0″·36, and that Mr. Stone had not given the slightest indication as to which formula he had adopted. If Mr. Stone had stated clearly in 1867 the corrections which he had applied to the Greenwich results, it might have been possible to verify his work. As it is, I must protest against the circumstance that discordances are still left between Mr. Stone's results and mine being taken as evidence of inaccuracy on either part. As regards my own work, I have, since the publication of Mr. Stone's paper, carefully recomputed the residuals, and have obtained identical results. The only changes I have made have been to transpose β *Ursæ Minoris* to the group 15°–30°,* θ *Ursæ Majoris* to the group 37° 40′–43°, and f_1 *Cygni* to the last group 43°–45½°.

I may add that I have not applied in this portion of my work corrections for error of the thermometer, as there is some uncertainty about the amount in individual years.

But, notwithstanding Mr. Stone's explicit statement in the *Monthly Notices* for April 1880, pp. 338, 339, of the corrections which he has applied in forming his last residual, there are several points to be cleared up:—

1. Corrections for errors of thermometer and barometer are stated to have been applied to the observations in 1857 and part of 1858. Now, I do not find in the Greenwich "Introductions" any observations in those two years which would fall in this group.

2. The limits of Mr. Stone's last group are N.P.D. 43° to 46° 29′. I have not been able to find any observations in the Greenwich "Introductions" between N.P.D. 45½° and 47½°.

3. The assumed co-latitude is stated to be 38°31′ 21″·80, but in his paper of 1867 Mr. Stone, after applying a correction —0″·205 obtains 38° 31′ 21″·61, which would imply an assumed co-latitude 38°31′21″·815. The difference is trifling, but it must be remembered that Mr. Stone gives his residuals to a thousandth of a second.

I call attention to (1) and (2), not in any spirit of criticism, but because there seems a possibility that as Mr. Stone extracted his results from the Greenwich Annual Catalogues, his last group may contain observations accidentally omitted from the Latitude Investigations in the Greenwich "Introductions" which I used.†

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* In discussing the observations 1851–61 and 1862–65 I had taken 15½° as the limit of one of my groups, and had at first used the same limit for 1857–65. The correction only amounts to 0″·02.

† On examining the *Greenwich Observations* for 1857, I find that there are, as a matter of fact, several stars in the Annual Catalogue which, for some reason, are not included in the Latitude Investigation in the Introduction. Mr. Stone had not stated in his paper of 1867 that he had taken his results from the Annual Catalogues, and I thought it more natural that he should have taken them from the Introductions where they are collected. Thus our data are not the same.

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On the rejection of the reflection observations by Mr. Stone I wish to offer a few remarks, not on account of its effect on the matter at issue, which is probably insignificant, but because it seems to be opposed to a principle usually accepted by astronomers—viz., that where an instrumental correction is determined by observations of a number of stars, the observation of each star may be used as a determination of its place, notwithstanding the fact that the observation in question has contributed *in a small degree* towards the determination of the instrumental correction on which the determination of place depends. Thus when the clock-error is deduced from transits of a number of clock-stars, the transit of each clock-star is used in connection with the clock-error to determine the R.A. of the star. Now, during the period 1857–1865 the zenith points were determined from observations extending over a fortnight, and including usually more than a dozen stars observed by reflection in each group. Thus the N.P.D. from the reflection observation of any one star would be virtually independent of that from the direct observation, since this star had only contributed in a small degree towards the determination of the zenith point. Again, for the R—D correction, two constants are determined by observations of a large number of stars, and the argument applies, therefore, with even greater force.

But, as regards the residuals, there is a point of some importance on which I am at direct issue with Mr. Stone—viz., the estimated probable error of the result for the last group. Mr. Stone obtains a weight of 28 or 24 with a probable error 0″·2, while with the same system of weights, I obtain a weight 6·5, with a probable error 0″·4. I have for the last group taken the probable error of an observation above the pole as 0″·5 and S.P. as 1″·5, and I have altogether 30 observations above pole and 17 below, which gives as an *inferior* limit to the probable error 0″·38, on the supposition that they all referred to the same star. It is just possible that the additional observations included by Mr. Stone may account for the discrepancy.

In any case there are, I believe, but few stars observed in this group; and where the same star—*Capella*, for instance—is observed every year on the same division of the circle, account ought to be taken of the probability of a systematic error * affecting the series. Thus the weight ought not to be assumed to be the sum of the weights for the separate years. Unless Mr. Stone has followed this last and erroneous system, I hardly see how he can have got such a large weight and small probable error for the result of his group.

I now pass on to the general question of Mr. Stone's proposed alterations in Bessel's Refractions.

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* Such as division error or error of the micrometer screws. I have shown later on that this last error would affect the residual for *Capella* in the group 1868–1876 by 0″·18. And there may be unknown sources of error which would have a still larger effect.

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First, as regards the value of the constant of refraction:—

I gather from Mr. Stone's paper on the whole that he gives up the diminution of the mean refractions which he advocated in 1867; but he accompanies this concession by criticisms directed against the validity of the evidence in favour of Bessel's constant of refraction.

In the first place, he brings forward his discussion of the observations 1857–1865, and appeals to his residuals, and to the accumulated evidence of the Greenwich circumpolar observations in favour of his diminution of the mean refractions; while he calls attention to the very slight evidence on which Bessel's Refractions have been reintroduced. Now, it seems to me that Mr. Stone's alteration in the refractions was based on very inadequate data, and I cannot see any reason for his confining his attention to this particular period of nine years from 1857 to 1865. In his recent paper he has attempted to supplement it by a discussion of the period 1868–1876; but even then he only takes 18 years out of 42, and the last 9 years on which he relies do not lend much support to his views. So that it is really quite misleading to speak of the accumulated evidence of the Greenwich observations, when no account is taken of the 21 years of observation from 1836 to 1856 which tell against Mr. Stone's hypothesis. In the four periods, 1840–1847, 1851–1861, 1862–1865, and 1868–1876, which I have discussed, the first two support Bessel's Refractions, whilst the third and fourth show such large errors on either hypothesis that no certain conclusion can be drawn from them. The diagrams which I have given (*Memoirs*, vol. xlv. p. 161) show this at a glance much better than mere figures.

But Mr. Stone's hypothesis is undoubtedly supported by the circumpolar observations 1857–1865. His residuals for 1857–1865, with the diminished refractions, are:—

−0″·15   +0″·10   +0″·14   +0″·08   −0″·10;

or as I find them, with the law $b \sin z \cos^2 z$ for R—D,

−0″·37   +0″·02   +0″·10   −0″·25   −0″·14;

and with the law $b \sin z$,

−0″·33   +0″·20   +0″·37   +0″·07   +0″·22.

Against these we have to set the residuals, with Bessel's Refractions unaltered, for 1851–1861:—

−0″·10 +0″·02 +0″·04 +0″·27   +0″·15   +0″·04 +0″·02 −0″·02;

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and for 1840–1847,

+0″·15 +0″·05 −0″·07 −0″·18 −0″·07 +0″·02 +0″·37 +0″·20,

these last being taken in connection with Mr. Main's discussion of the observations below Z.D. 70° (*Memoirs*, vol. xxvi.).

These residuals compare favourably with Mr. Stone's, so that we have 19 years in favour of Bessel's Refractions, as against 9 years only in favour of Mr. Stone's alteration. And it is to be remarked that Mr. Stone has only formed five groups, thus simplifying the problem of obtaining small residuals. For with two quantities—viz., the co-latitude and the constant of refraction—at our disposal it is much easier to satisfy five equations than eight. But the mere circumstance that the residuals are small gives no indication by itself that the solution is deserving of confidence. In illustration of this, let us take the residuals from the Melbourne circumpolar observations 1863–1865, discussed by Mr. Stone in his paper of 1867. From these Mr. Stone deduces a correction of +0″·15 to the co-latitude, and of −0·00372 × refraction to Bessel's Refractions, and he adopts these without hesitation. But, after all, it appears that Bessel's Refraction undiminished, with Main's correction below Z.D. 82°, gives smaller residuals.

Mr. Stone's residuals for (S.P.D. above Pole — S.P.D. below) are:—

N. P. D.	180°–170°	170°–160°	160°–155°	155°–150°	150°–147°
	−0″·23	+0″·20	+0″·44	−0″·03	−0″·39;

while Bessel's Refractions, with correction +0″·50 to co-latitude, give:—

	−0·01	+0·26	+0·27	−0·59	−0·38
Weight	122	26	8	8	11.

Multiplying the two sets of residuals by √(Weight), so that each has a probable error corresponding to weight, we have them in a form more fitted for comparison:—

Mr. Stone's Refractions	−2″·54	+1″·02	+1″·25	−0″·08	−1″·29;
Bessel's ,,	−0·11	+1·33	+0·76	−1·67	−1·26.

Thus, on the whole, Bessel's Refractions give more accordant residuals, the greatest error corresponding to the algebraic sum of 1 observation above and 1 below pole, being 1″·67 with Bessel's Refractions, and 2″·54 with Mr. Stone's solution. This is also seen on applying the usual criterion of least squares, for the sums of (residual)^2 × weight are 10″·72 with Mr. Stone's

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solution and only 6″·72 with mine. And yet *per se* Mr. Stone's residuals seem to leave little to be desired. If Mr. Stone had in this and in other cases given the probable error of his determination of x and y, the untrustworthiness of the solution would have been evident.

Somewhat similar criticism applies to the treatment adopted for the Greenwich circumpolar observations 1868–1876. The bulk of the observations are, as in the Melbourne results, included in the first two groups with vastly preponderating weight, and the correction to the refractions is sought to be established by means of two groups with small weights. The last group, 43°–46½°, formed by Mr. Stone contains 11 stars with total weight 34, exactly equivalent as regards number of stars and weight to each one of the 36 groups of 11 stars given in the Introduction to the Nine-Year Catalogue, p. 13. Of these 36 groups 23 are lumped together by Mr. Stone in a single group with weight 930, while the "all-important" last group has, as just mentioned, a weight 34 only. The degree of accuracy to be expected in a residual with this weight may be seen by examining the means of groups with corresponding or greater weight in the Introduction to the Nine-Year Catalogue, or the dots which represent these means in the diagram given in my paper in the *Memoirs.* If dots corresponding to Mr. Stone's last two groups be inserted on the diagram the difficulty of determining any correction to the co-latitude or to the refractions from the 38 dots here laid down will, I think, be manifest. But as Mr. Stone has grouped the residuals, the accidental errors are, to a great extent, masked, and the omission of the weights leaves us without any indication of the relative values of the several groups. Thus, presenting only five groups, he, by means of an arbitrary correction to the co-latitude, obtains small residuals, to which he appeals in proof of the soundness of his hypothesis. The only satisfactory way to test this, however, is to compare it with the results given with Bessel's Refractions, applying, as I have done throughout, Main's correction below Z.D. 82°.

Thus we have, taking ten groups corresponding to those in my paper on the Greenwich N.P.D.'s. :—

N.P.D. above Pole — N.P.D. below, 1868–1876.

With Mr. Stone's diminished Refractions and Co-latitude 38° 31′ 21″·60.

−0″·21 +0″·18 +0″·28 +0″·76 +0″·28 +0″·24 +0″·28 +0″·52 +0″·72 +0″·19;

With Bessel's Refractions and Co-latitude 38° 31′ 21″·90,

−0″·10 +0″·28 +0″·35 +0″·75 +0″·12 −0″·02 −0″·07 −0″·08 −0″·34 −0″·61.

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In the latter part of his paper (*Monthly Notices*, vol. xl. p. 346), Mr. Stone gives for the group N.P.D. 41°-46½° with the diminished refractions and co-latitude 38° 31′ 21″·60, the residual +0″·71. With Bessel's Refractions and the same co-latitude, he gets −1″·33. To this must be applied a correction of +0″·60 to reduce to co-latitude 38° 31′ 21″·90, and a further correction of +0″·60 on account of Main's correction to refractions below Z.D. 82°. Thus with Bessel's Refractions as corrected by Main, we have the small residual −0″·13 to set against Mr. Stone's residual +0″·71.

In this case the observations to which Main's correction is applicable have a total weight of about $\frac{4}{7}$ of that for the whole group. I have taken the mean value of Main's correction to this group as—1″·06 corresponding to mean N.P.D. 45° S.P. Thus for the three groups at large Z.D., Mr. Stone's diminution of Bessel's Mean Refractions gives somewhat larger residuals. Mr. Stone, of course, reduces his residuals by an appropriate correction to the co-latitude; but I would submit that this is not permissible. The annual results of the Greenwich observations show that the latitude has not changed in the course of more than 40 years, as Mr. Lewis Boss has pointed out in his memoir on the "Declinations of Stars" (p. 37),* and as may be seen from the results for co-latitude which I have collected in my paper (*Memoirs*, vol. xlv. pp. 156 and 167). So that we are not at liberty to assume one value for the observations 1857–1865, and another value for the observations 1868–1876.

As regards the observations of the Sun, I have fully discussed them for the whole period of 44 years, and I need not, therefore, further refer to the partial results which Mr. Stone adduces.

I now come to the comparison of the Greenwich observations with those at the Cape and Melbourne.

With respect to Melbourne, Mr. Stone, I think, does well to point out the difficulty as regards the large correction to the refractions north of the zenith, which he introduced (as he expresses it) "to force a close agreement between the results." But the influence of these on the determination of the refractions at Greenwich would have been clearer if he had given also the comparison with the Nine-Year Catalogue uncorrected. This comparison I now give, and it will readily be seen how far we are arguing in a circle:—

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* I regret that this important work did not reach me till after my paper was read, so that I could not make reference to it as I might otherwise have done.

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(Corrected for Error of Thermometer at Greenwich.)

N.P.D.	G'_6-M.	G_6-M.	G'_6-M'.	G_6-M'.
°	″	″	″	″
48	−0·37	−0·92	+2·33	+1·78
52	−0·25	−0·82	+1·67	+1·10
56	−0·04	−0·63	+1·41*	+0·82
60	+0·21	−0·41	+1·36	+0·74
64	+0·49	−0·16	+1·42	+0·77
68	+0·69	+0·01	+1·45	+0·77
72	+0·74	+0·03	+1·37	+0·66
76	+0·58	−0·16	+1·11	+0·37
80	+0·28	−0·50	+0·72	−0·06
84	+0·04	−0·78	+0·40	−0·42
88	+0·18	−0·69	+0·47	−0·40
92	+0·18	−0·74	+0·42	−0·50
96	+0·27	−0·71	+0·45	−0·53
100	+0·35	−0·72	+0·48	−0·59
104	+0·41	−0·77	+0·50	−0·68
108	+0·32	−0·99	+0·36	−0·95
112	+0·36	−1·16	+0·36	−1·16
116	+0·35	−1·51	+0·31	−1·55
120	+0·31	−2·15	+0·23	−2·23
122	+0·07	−1·84	−0·02	−1·93
124	+1·15	+0·33	+1·04	+0·22

G_6 = Greenwich Nine-Year Catalogue with Bessel's Refractions (1 − ·00532).
G'_6 = „ „ „ with Bessel's Refractions.
M = Melbourne Catalogue 1870, with Stone's Corrections.
M′ = „ „ without „ „

As Mr. Stone thinks he applied, in his paper of 1867, corrections for error of the thermometer in forming his residuals, though we have no direct evidence on the point, I have corrected the results for error of the thermometer at Greenwich (+0°·55) so as to put the case as favourably as possible for Mr. Stone. In my former paper I had not applied any such corrections to the Greenwich Catalogues, as there is some uncertainty with

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* Mr. Stone gives this residual as 1″·65, which is apparently a misprint for 1″·40.

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respect to the error of the thermometer in the case of three of them, and I should be inclined to say that Bessel's Refractions satisfy the Greenwich observations as they stand—*i.e.*, with an error of perhaps +0°·4 or +0°·5 in the thermometer.

I think it is clear that, whether we take the Melbourne N.P.D.'s, with or without Mr. Stone's corrections, the substitution of Bessel's Refractions for Mr. Stone's in the Greenwich Nine Year Catalogue greatly improves the residuals. And it is easy to distinguish between the effect of an alteration in the Greenwich refractions and the effect of an alteration in those at Melbourne. The former affects mainly the residuals at large N.P.D.'s, the latter those at small N.P.D.'s.

There can be no doubt that there is some systematic error in the Melbourne observations, increasing rapidly towards the north horizon,* and this must diminish our confidence in a comparison with this Catalogue. But it seems to me more probable that the discordances are due to some instrumental error than to a difference in the refractions north and south of the zenith. In the absence of detailed information on the flexure, R—D discordance, zenith points, and errors of the micrometer screws, it would be unsafe to suggest the cause or causes of systematic errors. It may be remembered that Professor Adams found a discordance amounting to 1″ in the results for nadir point of the Cambridge Transit Circle, according as the instrument had been pointing to the North or the South, and this was ultimately traced to a defect in the soldering of one of the ends to the central cube. This may suggest that we ought to take account of the possibility of discontinuous as well as continuous functions in the expression of instrumental errors, and that a discordance in the nadir point does not necessarily imply a correction of the form $a \cos z$ in the flexure. At Greenwich such a discordance was found to result from error in the micrometer screws, and to be peculiar to the nadir point observation itself. Thus we are not absolutely driven to the improbable supposition of large differences in the refractive power of the atmosphere at different places and on different sides of the zenith. The principle of forcing an agreement between observations by arbitrary alterations of latitude and refractions without much regard to physical facts does not seem very philosophical. By such a process we temporarily get over a difficulty, but do not get any nearer to the truth.

I am quite ready to admit that the comparison with the Melbourne observations alone would not be conclusive, but the Cape results seem to leave no doubt as to the substantial accuracy of Bessel's constant of refraction, as the following comparison shows:—

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* N.P.D. 48° corresponds to Z.D. 80° N. at Melbourne and to Z.D. 76° N. at the Cape.

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Greenwich Nine-Year Catalogue—Cape Catalogue (1860).

(*Corrected for Error of Thermometer at Greenwich.*)

N. P. D.	Bessel's Refractions.	Bessel's Refractions (1 − ·00532).
48°	+0″·88	−1″·46
52	+0·80	−1·28
56	+0·66	−1·26
60	+0·56	−1·27
64	+0·54	−1·22
68	+0·59	−1·12
72	+0·62	−1·06
76	+0·52	−1·14
80	+0·31	−1·35
84	+0·08	−1·58
88	+0·01	−1·66
92	+0·04	−1·65
96	+0·10	−1·62
100	+0·13	−1·66
104	+0·14	−1·73
108	+0·06	−1·92
112	+0·06	−2·10
116	−0·08	−2·56
120	−0·15	−3·21
122	−0·35	−2·85
124	+0·78	−0·62

In the second set of residuals the effect of using the diminished refractions at the Cape as well as at Greenwich is shown. A correction of −0″·58 has been applied to the co-latitude of the Cape Observatory (56° 3′ 56″·44) as representing the effect of the diminution in the refractions.

Thus in favour of Bessel's constant of refraction we have the circumpolar observations for nineteen years, the comparisons with the Cape and Melbourne, and the observations of the Sun for twenty-eight years; and in favour of Mr. Stone's diminution of the constant, circumpolar observations for nine years only, and observations of the Sun for fifteen years. And even for the limited period on which Mr. Stone relies, the residuals are largely dependent on the assumption of the law $b \sin z \cos^2 z$ for R–D. I have taken no account of the circumpolar observations in the periods 1862–1865 and 1868–1876, as their evidence is inconclusive.

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Secondly, with respect to the law of refraction :—It may be taken, I think, that Bessel's formula for the decrease of temperature in the atmosphere is not consistent with fact, and that this is the reason why his refractions for zenith distances beyond 80° do not accord with observation. This being so, I cannot see that anything will be gained by tinkering up Bessel's theory and distributing the errors in a different manner. Mr. Stone may easily get a tolerable agreement for the limited range of observations which he discusses—viz., 80° to 85° Z.D.; but his alteration would not satisfy a more extended series. Bessel's Refractions carry us very well as far as Z.D. 80°, and even perhaps to Z.D. 82°. Mr. Stone attempts to extend the range to Z.D. 85°, introducing, however, sensible discordances above Z.D. 80°. Is anything really gained by this? And how will observations below Z.D. 85° be satisfied? Bessel professed to satisfy the observed refraction at the horizon, and also secured a good agreement with observation as far as Z.D. 80°; Mr. Stone throws over all refractions below Z.D. 85°, and introduces divergences from other theories as well as from observation between 75° and 80°. I am prepared to admit that Bessel's is an artificial theory, and that his law of refraction is erroneous, though the constant satisfies observations; but there is a great convenience in retaining refractions which have been so generally used, until we have tables founded on a really satisfactory theory with which to replace them. Uniformity of system will greatly facilitate the determination of corrections. I have called attention in my former paper to the circumstance that Bessel's rate of decrease of temperature in the atmosphere is too small at small altitudes and too large at great altitudes. Laplace's hypothesis agrees much better with observations of temperature made during balloon ascents, and it makes the refraction at 85° smaller than Bessel's by 1″·93, which is fully two-thirds of the correction indicated by observation.

I do not propose to enter further on this question now. My reason for alluding to it is to point out that in order to secure an agreement at 85° there is no occasion to introduce comparatively large corrections to Bessel between 75° and 80° Z.D. The differences (Bessel—Laplace)* are only −0″·09 at 75° Z.D., and −0″·17 at 80° Z.D.; while those for Bessel—Stone are +0″·14 and +0″·46. As the differences between Lubbock's Refractions and Bessel's are equally small, I think we may safely assume that at Z.D. 80° Bessel's theory is not in error by more than 0″·2, which only amounts to ·0006 × refraction, or about $\frac{1}{1600}$th part.

This point has an important bearing on the determination of

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* Using Caillet's Tables (*Conn. des Temps*, 1851) calculated on Laplace's theory, and reducing to Bessel's constant of refraction and to temperature 50° F., I find the following differences (Bessel—Laplace):—0″·00 at Z.D. 45° and 60°; −0″·09 at 75°; −0″·17 at 80°; −0″·18 at 81°; −0″·12 at 82°; +0″·08 at 83°; +0″·61 at 84°; +1″·02 at $84\frac{1}{2}$°; +1″·93 at 85°.

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the constant of refraction. For it is clear that if we want to find the correction y to this constant within such limits as ·003, an error of ·0006 in Bessel's theory is of no practical importance, and thus we extend our range of Z.D. to 80°, and double the effect on our residuals. Even Mr. Stone's discordance from Bessel's theory of $+0''\!\cdot\!46$ would only imply an error of ·0015 × refraction; so that on his own showing there is no occasion to increase the difficulties of the problem by restricting the range to 75° Z.D.*

But the case is very different beyond 82° Z.D., where the discordances between different theories begin to increase rapidly in proportion to the refractions. It seems to me, then, that we may safely use Bessel's Refractions as far as Z.D. 80° for the determination of the constant of refraction, since the error of theory is but a small fraction of the uncertainty of the constant; and that beyond 82° Z.D., the conditions being reversed, observations may be used for determining the law of refraction, on the assumption that the constant is known with tolerable accuracy.

Thus for the determination of y from circumpolar stars, instead of Mr. Stone's limiting equations

$$x + 90y = a_1, \qquad x + 215y = a_m,$$

we have

$$x + 90y = a_1, \qquad x + 313y = a_n\,;$$

and from results at northern and southern Observatories, instead of

$$x + 116y = b_1, \qquad x + 230y = b_m,$$

we have

$$x + 116y = b_1, \qquad x + 316y = b_n\,;$$

and an error of ·003 in y will introduce into our residuals a_1 and a_n a difference of about $0''\!\cdot\!65$ instead of $0''\!\cdot\!35$.

I have taken Mr. Stone's limiting equations merely for the sake of comparison between the limits 75° and 80° Z.D. They are a trifle too favourable to the circumpolar observations, and perhaps hardly fair to those at northern and southern Observatories. We ought, I think, to assume some such equations as these:—

$$x + 92y = a_1, \qquad x + 313y = a_n,$$
$$x + 75y = b_1, \qquad x + 316y = b_n,$$

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* I must admit that on the second page of my paper (*Memoirs*, vol. xlv. p. 152) I have spoken of Z.D. 75° as the limit at which the law of refraction begins to get somewhat uncertain, but on the next page and subsequently I have taken 78½°, and finally I have concluded that Bessel's Refractions are sensibly correct as far as Z.D. 80°. In explanation of these apparent inconsistencies, I may remark that the various theories of refraction *begin* to diverge about Z.D. 75°, but the divergence in the case of Bessel's, Laplace's, and Lubbock's Refractions is of little practical importance till beyond Z.D. 80°.

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the sum of refractions for *Polaris* above and below pole being 92″, and the refraction for a star at N.P.D. 81° equidistant from the zeniths of Greenwich and the Cape being 53″. I have taken the coefficient of y as $53\sqrt{2}$, assuming the probable errors of the determinations of y and z (the correction to the constant of refraction for the Cape) to be the same. I do not, however, insist on this point, as the actual problem consists in the determination of *three* unknown quantities from a system of equations with different weights, and seems hardly to admit of statement in the simple form of two limiting equations with only two unknown quantities.

The greatest difference of residuals from circumpolar observations would be $221y$, whilst from northern and southern Observatories we should have $241y$.

These equations give, according to my view, a fair comparison of the relative value of circumpolar observations and of results at northern and southern Observatories for the determination of y, on the supposition that we are at liberty to assume different values of y for Greenwich and the Cape—an hypothesis which I am not prepared to admit without further evidence.

But, as I have shown in my paper on the Systematic Errors of the Greenwich N.P.D.'s, the case is far more favourable when we assume y the same at the two Observatories. For the satisfactory comparison, however, of observations at Greenwich and southern Observatories, we require a thorough discussion of the latitude, flexure, R−D correction and errors of micrometer screws for the Cape and Melbourne.* Reflection observations of stars extending over a large range of Z.D. are particularly wanted at Greenwich, as well as at the southern Observatories, so that the R−D correction may be determined from a curve, and not from an arbitrary law. We may hope in due course to have these data, and then we shall be able to make use under the most favourable conditions of the comparisons of the results at northern and southern observatories.

In discussing the circumpolar observations for the group Z.D. 80° to 85°, 1868–1876, Mr. Stone makes a remark (*Monthly Notices*, vol. xl. p. 345) which, I think, must have been written in momentary forgetfulness of the system which has been in use at Greenwich for upwards of 40 years, including the period during which he himself was there. I allude to the system of making the determinations virtually dependent on observations of stars alone. Mr. Stone says: "The co-latitude during the second period [1868–1876] appears to have been affected by the constant difference which existed during the period 1870–1874 between the nadir point determinations, and the virtual rejection of the observations with the reflecting eyepiece, by the application of constant corrections, to make their results agree in mean results

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* In the *Monthly Notices* for 1879 December Mr. Stone has given the results of a discussion of the latitude of the Cape, which afford valuable information on this point, and we may hope for fuller details when the Cape Catalogue for 1880 is published.

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with the zenith points found from the star observations. To some extent, however, any error which might arise from the course adopted will be compensated for when the co-latitude obtained from the observations themselves is adopted, as in the present comparison."

Now, it is well known that since 1836 the practice at Greenwich has been to deduce the R—D correction from observations of stars alone, and to include in it a constant term which represents the correction to adopted zenith points to make them agree, in mean results, with those found from the star observations. It is clear that it can make no difference in mean results for a year, whether we apply the correction at the end of the year to make the adopted zenith points agree with those resulting from stars, or whether we apply the requisite correction to each nadir point observation to make its results agree in the mean with those given by stars. The latter course was adopted in the years 1871–1874, and that it was virtually equivalent to the constant correction included in R—D in 1869 and 1870 is shown by the smallness of the constant term in R—D after the correction to the nadir point observation had been applied.

Mr. Stone's language would imply that the source of the discordance in the nadir point observation is unknown, and as its existence would then cast discredit on the results 1868–1876, it may be well to make a few remarks on it here, though I have already explained its origin in the errors of the micrometer screws in a former paper (*Monthly Notices*, vol. xxxvii. p. 18).

In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at 0^{r} of the microscope micrometers, and thus was obtained that accordance of separate results which is so dear to some astronomers, but which is not always conducive to the elimination of systematic errors. The additional wear resulting from this increased use of one part of the screws gave rise to a discordance between the nadir observation and observations of stars which were taken at all parts of the screws. This discordance is shown in the years 1869 and 1870 by the large constant term in R—D; in subsequent years a correction was applied to each nadir observation derived from the mean results for the preceding year. In 1875, being dissatisfied with the large correction for runs virtually required to reduce the nadir observation to the mean of observations of stars which would fall at about $2^{\mathrm{r}}\cdot 5$, I suggested that the nadir observation should be taken at from $2^{\mathrm{r}}\cdot 5$ to $3^{\mathrm{r}}\cdot 5$, and it was then found that the correction to the nadir observation had changed from $-0''\cdot 8$ to $+0''\cdot 6$. This threw suspicion on the micrometer screws, and their errors were examined, as explained in my paper of 1876. The result was to show that corrections were required ranging from $-1''\cdot 4$ at $\bar{1}^{\mathrm{r}}\cdot 3$ to $+0''\cdot 8$ at $2^{\mathrm{r}}\cdot 3$, the correction at $0^{\mathrm{r}}\cdot 0$ being $-0''\cdot 9$, corresponding very closely with the correction to the nadir observation $-0''\cdot 8$. Further, observations taken for another purpose in 1868 showed that the errors of the micrometer

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screws were sensibly the same at that time, with the exception of the portion about $0^{\mathrm{r}}\cdot 0$, which had suffered extra wear since 1868 from a preponderance of some 4000 observations taken at this part. On inspecting the threads of the screws they were found to be much worn, as indicated by the errors. Thus there is no mystery about the discordance of the nadir observation, and there is no need to assume a term $a \cos z$ in the flexure. The apparent change in the co-latitude on which Mr. Stone remarks has nothing to do with this correction, but is simply due to a change in the system of weights given to the close circumpolars and to the increased number of observations of wide circumpolars. It did not begin in 1871, when the correction to the nadir observation was first applied, but in 1872, when special attention was directed to the observation of wide circumpolars. Previous to that the large weight given to the four close circumpolars made the value of the co-latitude virtually depend on them, and if this system had been continued the same value would have been obtained in subsequent years. The four close circumpolars give for the period 1868–1876 $38^\circ\ 31'\ 21''\cdot 95$ with Bessel's Refractions, and $38^\circ\ 31'\ 21''\cdot 70$ with the diminished refractions; whereas the stars about N.P.D. 22° give about $38^\circ\ 31'\ 21''\cdot 50$ and $38^\circ\ 31'\ 21''\cdot 17$. It is simply the admission of these latter stars to a fair share in the result which has produced an apparent change. This apparent change does not justify Mr. Stone in using two different co-latitudes in the two periods 1857–1865 and 1868–1876.

Mr. Stone has done good work in collecting the results for north refraction stars; but before making use of them it is important to know whether he has applied the corrections for errors of the micrometer screws. In the case of *Capella*, which has the largest weight of any star in the group, and which has been observed in every year, I find that the correction applicable on this account to Mr. Stone's "Excess of N.P.D. below Pole" amounts to $-0''\cdot 18$, and the corrections for other stars may easily be larger. The necessity for similar corrections in the period 1857–1865 suggests the exercise of caution in accepting estimates of probable error based simply on the number of observations, even when the possibility of a small outstanding division error is included. The corrections to the residuals in 1874 for λ *Ursæ Minoris* (47 observations), *Polaris* (152 observations), *Cephei* 51 (60 observations), and δ *Ursæ Minoris* (57 observations), on account of error in the micrometer screws were $-0''\cdot 11$, $+0''\cdot 25$, $-0''\cdot 52$, and $+0''\cdot 39$ respectively. Corrections of such magnitude to the mean results of a large number of observations would hardly have been expected.

Blackheath,
1881, *January* 12.